THE POLITICS OF CANCER[1]

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. . . cancer is not simply an island waiting in isolation for a crash program to wipe it out. It is in no way comparable to a moonshot – to a Gemini or an Apollo program – which requires mainly the mobilization of money, men and facilities to put together in one imposing package the scientific knowledge we already possess.
Instead, the problem of cancer – or rather the problem of the various cancers – represents a complex, multi-faceted challenge at least as perplexing as the problem of the various infectious diseases . . . We have barely begun to perceive the fantastic array of causative factors involved in cancer, the methods by which they work, and the agencies by which they may be controlled. We are not yet ready to start a count-down for an anti-cancer blast-off, no matter what emotional appeal such an approach may have to the public.

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Internal NCI affairs are very complex, and outsiders comment on them at their peril because there are so many political issues involved.

Howard M. Temin
Nobel Laureate

Introduction

Policy is "essentially an ordering of priorities."[2] Policy research purports to contribute to the ordering. Science policy researchers who so choose can cast aside their naivete and plunge, as Temin suggests, into the world of biomedicine. The choices, however, do not end there. For the sociologist of science who simply seeks to understand the effects of biomedical research policy in the United States on the development of research (perhaps hoping

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in the future to offer advice on the ordering or reordering of priorities), a second choice must be made. How close does the sociologist dare to get to the politics of a dreaded disease? What kinds of information does one deem to be germane to an assessment of current and past priorities, outcomes, and mechanisms of control? How, in short, can the sociology of biology contribute to policy decisions?

Such choices must necessarily be made prior to the assessment of empirical study itself. The choices of sociologists of science, however, have been virtually unanimous: keep your distance and adopt a framework that is socially impeccable – attentive to organizational structures, institutionalization, and the ethos of science – but impervious to intellectual content. Another choice, one that sociologists have spurned or avoided altogether, namely, the use of the structure of argumentation of a field to scrutinize the efficacy of its policies and their relation to the social setting of new research priorities, seems to be more realistic and helpful for the scientist and policy-maker.

But how do policy decisions about funding, for instance, affect this scientific argumentation process? As we shall presently show, biological scientists seem to perceive the structure of their research problems, and to criticize funding policy from this perspective. What science would have been like had certain policies not been put into effect, one will never know; but the sociologist can attempt to understand the political forces that shaped the scientific problems that *are* studied.

The study of science policy is, of course, a subject in itself. The more limited goal here is to observe the critical discussion surrounding the early stages of the war on cancer. Specifically, we seek to raise sociological consciousness about the rhetoric and organization of science as they impinge on biomedical research policy, funding, and the disposition of knowledge.

The Context for Assessment

U.S. medical research policy, as was evident by the late 1940's "was not going to be established by hard consensus on a grand design. It would be fragmentary and incremental; in short, evolutionary."[3] The evolution of the cancer research policy[4], however, was interrupted by the passage of the National Cancer Act in 1971.[5] With the influx of monies into cancer-related research[6] the evolutionary course of basic cell transformation research was to be profoundly altered.[7] The prospect of a National Cancer Authority,

envisioned in Bill S.34 as separate from NIH and possibly administratively independent of the National Cancer Institute, had generated scientific, organizational, and political cross currents that were seldom found in earlier, small scale biological research. As some (notably, Weinberg[8]) had predicted, the time had come for Big Biology: in the past Big Physics met with success, so now the national funding focus must shift to the biological sciences.

The major contemporary impetus for expanding cancer research was heralded by former President Nixon's second State of the Union Message:

> I will also ask for an appropriation of an extra $100 million to launch an intensive campaign to find a cure for cancer, and I will ask later for whatever additional funds can effectively be used. The time has come in America when the same kind of concentrated effort that split the atom and took man to the moon should be turned toward conquering this dread disease. Let us make a total national commitment to achieve this goal.[9]

This request elicited an overwhelmingly positive response. Cancer as a symbolic threat was seemingly capable of unifying both Congress and the electorate (as Vietnam had not). In turn, it was difficult for scientists to find fault with this increased subsidy of biological science, given the broader context of slackening governmental support of science.[10]

Even so, some reservations about the funding of cancer research were subtly expressed. Dr. Campbell Moses, medical director of the American Heart Association, noted before the Senate subcommittee on health,

> . . . that if the state of research in cancer makes reasonable such a comprehensive effort to the control of cancer today, an exactly parallel effort is even more appropriate in the field of heart disease. In the cardiovascular field, we know that the expansion of our research effort, and the comprehensive full–scale application of the fruits of already available research and technology, would save lives now.[11]

There resides in these remarks an attitude that the cardiovascular field (which studies the number one killer, heart disease) is ripe for research, riper in fact than cancer. Political symbolism and scientific investment priorities seemed, at least to some, confused. But while such reservations are implied, there was also an unmistakable endorsement of the principle of funding the cancer program. As Senator Edward Kennedy remarked at the close of this testimony, "There are those who say if you can't get a raise yourself, the best thing that can happen is for the fellow next to you to get a raise."[12]

So while most scientists were reluctant to criticize an allocation to the "fellow next to them" – there was tacit agreement that Big Biology was needed and cancer was a strategic site – reservations centered on research *organization.* How was Big Biology to be organized? Although sociologists tend to think about organizational structures in terms of vested interest, power maintenance, or political gain, what emerged was researchers' concern about how the Cancer Act was to be administered and how this related to the development of biomedical science. Whether it be administered autonomously as a NASA–type special project or as part of the existing NIH organization was more an issue of research styles and reasoning processes within biomedicine than one of maintenance of role relations and political allegiances. If one takes at face value, at least provisionally, the scientists' expressed concerns, the real issue in the Cancer Act is how biomedicine should interface with organization. Aside from the inevitable charges of favoritism and the abuse of power (both discussed below), the scientists' rhetoric centered on the hindrance of the advancement of knowledge. What is at issue was the cognitive orientation toward research mobilization which the biomedical scientists shared, and which must be characterized if one is to understand their critical posture toward organizational imperatives,[13] e.g., new funding mechanisms.

The biomedical researcher in the 1960's had already witnessed the pressures toward bigness and the scientific difficulties with mission-oriented research. As HEW Secretary John Gardner cautioned in 1966:

> The question remains whether ... we should mount large-scale highly organized applied research or developmental projects with specified short term goals. The answer is 'yes'. But in giving that answer we must bear in mind that each such effort is apt to be extremely expensive ... And we must not imagine that dollars and large-scale organization are an adequate substitute for ideas and a sound scientific base. Where the ideas and the scientific base do not exist, it is possible to waste vast amounts of money under the banner of practicality.[14]

This caveat was echoed with even greater intensity by former (1955–1968) NIH Director Shannon in whose view, according to Strickland,

> Targeted research, research aimed at finding cures for particular health problems, was ... not only the most expensive but certainly the most wasteful kind. The waste was not limited to dollars, but included use of scientific energies, for research efforts narrowly aimed at single targets could restrict the beneficial effects of the internal dynamics of science.

> Moreover, for NIH to place too much emphasis on directed research would be to retard the development of science in another way: it could artifically skew the production of new medical scientists.[15]

Throughout these discussions it was the "scientific base", the "internal dynamics of science", that was being used as the rationale for organizational structure.

In the 1970's, again many scientists recognized that the conquest of cancer was not a NASA-type operation because the problem to solve was essentially a nontechnical one.[16] The Association of American Medical Colleges' testimony before the subcommittee, presented by John A.D. Cooper, president of the Association, illustrates this point:

> The Manhattan Project and the space program have been cited as successful precedents for the establishment of independent mission-oriented agencies. However, ... harnessing the atom and the space program were largely technological challenges built upon a firm base of fundamental scientific knowledge. Their targets, though far away, were defined. In the Cancer Conquest Program the targets are diffuse, unseen and largely unknown.[17]

The argument is clear: the intellectual underpinnings of cancer research are radically different from the usual input–output model of purely technological programs. If the technological model works, it works because the "black box" interposed between the inputs and the outputs is well known or imminently knowable through some directed effort. In the case of cancer research, the contours of the black box are sufficiently vague that it is difficult to decipher where *not* to invest.

How, then, are research investments made (in the guise of policy strategies)? Surely there is disdain, expressed here by Cooper, for the nonscientists' view of how scientific answers materialize:

> An unfortunate misconception apparently is developing that the mere injection of additional federal cancer research funds will produce somehow an instant cure for cancer. Its equally misleading corollary is that the key to the conquest of cancer – one of the grimmest and most intractable groups of diseases that besets the world's scientists – lies in the managerial efficiency and the capacity of the medical-industrial complex.[18]

Other excerpts from his address reiterate the point:

> There is no instant cure [for cancer]. And to imply that money can buy one is as unconscionable as it is to suggest that the key problem is managerial rather than scientific . . .
> There is much that is uncertain in the field of cancer. But one fact is clear: The mere size of the national investment in cancer research is not the substantial determinant of speed in the conquest of cancer . . .[19]

The stark contrast of the views of Benno Schmidt, who chaired the National Panel of Consultants on the Conquest of Cancer, forerunner of the existing President's Cancer Panel, with those expressed above show that the organizational debate is not over.

> The valid analogy is not the scientific analogy but the organizational analogy. The cancer program, in order to succeed, needs the same independence in management, planning, budget presentation and assessment of program that those programs needed . . .[20]

It might just be that Big Biology demands organizational structures that, because of the "state of the art" and the nature of the discovery process(es), diverge from ordinary organizational principles in large scale research. That is, sociologists may have too hastily dismissed the power, and needs, of scientific idea structures as formative agents of organizations. But does this not capture the sentiment of the biomedical community, i.e., give us Big Biology but not mission-oriented research? The problem seems to lie in the structuring of problems and intended solutions. The Nobel laureate and NCI critic, James Watson, states

> . . . that high-quality cancer research is likely to be much more difficult to pull off than most other forms of biology . . . [W]e may not have even one really hot clinical lead that has a good chance to lead somewhere soon with a major cancer. So we must be much more careful than we have in the past as to what we allow our lobbyist friends to claim for us . . . We should do the science we are trained for and not hold the carrot too close . . . But if we respond to the fear of less cancer money for next year by flashing out even shakier new leads, say, in tumor immunology, to mask the fact that we still have not made the big breakthrough, we have nowhere to go but down.[21]

On the one hand, such statements reflect the tremendous pressures on the biomedical community to effect a cure for cancer. This can be seen in the

type of question that the present Senate health subcommittee (chaired by Edward Kennedy) continues to pose to the President's Biomedical Research Panel: "Why don't you people in the NIH and the medical schools spend less time 'understanding' disease and more time preventing or curing it?"[22] On the other hand, Watson's statement reflects the peculiar nature of biomedical progress, namely it is difficult to predict where a "breakthrough" will occur. Finally, it reflects the "uneasy partnership"[23] between government and science. The "fear of less cancer money for next year" elicits a public relations response from the scientist. The response from the scientists *must* be that progress is being made.[24]

The separation of imposed social structures from the structure of biomedical progress seems to be the source of the scientists' concern. If this is the nexus of the policy problem for the biologist, then it would seem to be the most crucial site for sociologists to analyze. To assess rationality of the biomedical research effort the sociologist must question scientists' complicity with the organizational-managerial orientation to research criticised by Cooper. For cancer, of course, is much more than an area of scientific research; it is a highly visible symbol and thus peculiarly vulnerable to political abuse. The growth in the number of cancer victims, the aging of the voting population pyramid and its possible partisan manipulation, growing concern over whether science could structure itself, and the need for conspicuous investments in science in the face of its dwindling public image and proportion of the GNP, all capture something of the political tensions pervading cancer research. Cancer as a unifying symbol provides a basis for political mobilization that perhaps can be exceeded only by issues of national defense. Again a statement by Cooper is apropos:

> In an ideal world, the Association would say there is no need for new legislation to carry out a new scientific offensive against cancer. But the situation being what it is, there clearly is going to be some legislation.[25]

That "situation" was charged with political overtones, forces with which scientists were ill at ease. Although Congress was ultimately convinced of the necessity for continuing the new cancer program within the structure of NIH, a compromise of S.34 in the form of Bill S.1828, the *internal* politics of cancer remained tense.

The political, organizational, and scientific components of cancer research represent an interesting example of the alienation of the process of science from the broader social milieu which supports it. Neither science nor society can afford to ignore the difficulties of mission-oriented Big Biology. When a

Sloan-Kettering virologist in the wake of the Summerlin "mouse painting" affair can say, "I have some advice for young researchers in biology. Stay out of cancer research because it's full of money and just about out of science,"[26] it is cause for concern.

Scientific Organization and Organizational Criticism

The demands of science, organization, and politics were and remain intimately intertwined in the campaign to conquer cancer. And the effectiveness of any evaluation of a program such as this demands that the various ingredients receive their due proportion of credit and blame. As one might expect after more than four years of the program, a lot of credit and blame is available. Daniel Greenberg has leveled severe criticism at the optimistic claims of finding a cancer cure that emanate from the National Cancer Institute, presumably seeing such claims as politically motivated and statistically suspect. The conservatism of the structure is also singled out for attack:

> My next visit was to the National Cancer Institute, where the official line is given on the record, but contrary views are offered only privately. 'It just doesn't serve to rock the boat,' a scientist told me. 'Look, when you've got 10,000 radiologists and millions of dollars' worth of equipment, you give radiation treatments, even if study after study shows that a lot of it does more harm than good. What else are they going to do? They're doing what they've been trained to do. Like surgeons. They're trained to cut, so they cut.' And research on prevention? 'It's picking up a bit after all these scare stories,' he said. 'But the level is actually a joke.'[27]

Has this conservatism of training, which for Greenberg includes a conservatism of treatment, as well as beliefs regarding the etiology of cancer (as implemented in the Special Virus Cancer Program), stymied research progress? Even if it has not, Greenberg's criticism highlights the subordination of scientific knowledge to organizational structures, suggesting that some research tactics of NCI are dead–end prone.

In 1974, some of the findings of the Zinder committee, which had been appointed by the Cancer Board,[28] were aired. The committee had been appointed after growing criticism of the Virus Cancer Program (VCP) indicated that an evaluation, not unlike the evaluation of NIH by the Woolridge committee a few years before, was in order. The Zinder committee's comments were generally harsh and pointed:

> First, the committee said, the VCP is too expensive. (It costs about $50 million to $60 million a year and consumes slightly more than 10 percent of the total NCI budget.) Second, the program must be opened up to the scientific community. At present, it is run by a handful of persons who have undue control over large amounts of money, which goes to only a limited number of laboratories. Furthermore, the individuals who award contracts are in a position to award them to each other, which somehow does not seem quite right. The committee called for new management practices and a good stiff measure of peer review by outside scientists.[29]

Several individual scientists dispensed enormous sums of money *annually*, notably $19 million to Huebner, $7 million to Todaro, and $12 million to Manaker. After reviewing VCP programs of research and their organizational structure, Zinder reported that "about 50 percent of the program is supportable at some level."[30] This severe criticism is partially based, like that of Greenberg, on intellectual investments of scientific administrators and researchers which preclude the convergences of ideas on other problems.

> Many of those in administrative control of the VCP are men whose careers are intimately linked to the idea that there is a relationship between certain RNA viruses and human cancer. Much of the research the program supports is aimed at substantiating this idea. VCP support of research on DNA viruses is comparatively small. The committee recommends . . . an integrated program with a built-in series of checks and balances to prevent the special notions of particular individuals from carrying the day. For example, should the first definitive [human] cancer virus turn out to be a papova virus [one of many suspected DNA viruses], the VCP would be in a strange position. It scarcely supports any work in this area and only recently has gotten seriously involved with the DNA viruses such as herpes.[31]

It is indeed difficult to moderate the tone of such criticism; its biting phrases are intended to elicit further evaluation of this portion of the biomedical research enterprise.[32] As such, it beckons for further evaluation and study of the Virus Cancer Program as it bears on viral cell transformation research and specifically the research centering around oncogenic RNA viruses.

How, then, should such "internal" criticisms be utilized by the sociologist of science? The criticisms certainly provide a baseline of interpretive evaluations of research in progress which sociologists can then approach from other perspectives; in short, they help generate hypotheses. But there would appear to

be an even more central role for criticism that incorporates the scientists' evaluations of organizational principles and practices. Critical comments on organizational structures mirror evaluations of the proper workings of science; they are statements about the state of science and the norms of reason. These norms of science have a function *within* the knowledge component of science. Unlike the traditional conception of the norms of science,[33] which supplants the knowledge function with an organizational maintenance function, a formulation is needed which restores to centrality the cognitive orientation of researchers. It should take into account the scientist's vocation as it becomes manifest both in knowledge and in organizational criticism. In short, a single set of institutional norms no longer suffices as behavioral guides in the context of Big Science. What other norms have scientists embraced? Some answers have already surfaced in our examination of the war on cancer. Now we turn to a more systematic analysis of these normative manifestations of Big Biology.

Interpreting Criticism: In Search of New Norms?

From 1971 to the present, the largest single biological research offensive that the U.S. has known has been directed by the National Cancer Institute. The funding of this institute relative to others within NIH has been the target of criticism[34] (some of which was recounted above). The criticism, however, flows not merely from disparities in allocations, but from disparate interpretations of the ethos underlying those allocations and the rhetoric of the scientific community to secure them and "ensure" the advancement of scientific knowledge. To reflect on this rhetoric is to speculate on its antecedents and to recognize its implications for alternative normative structures in the community.

To quote Nelkin:

> Merton's formulation [of the scientific ethos] was developed to reaffirm the values of science when it was faced with 'frontal assaults on its autonomy,' but assaults on science and its accepted values have become more vigorous, stimulated by growing perceptions of the importance of science to society and of its social consequences . . . The scientific community is ill-equipped to deal with external pressures. The norms of science [may] govern the behavior of scientists within their field, as if science were by definition an autonomous enterprise. But unlike physicians, or those in professional practice, scientists share no well formulated set of norms to govern their relationship outside the scientific community.

> The instinct to protect professional autonomy is backed by few rules that would guide an appropriate collective response. Thus, when unable to ignore persistent challenge, scientists often taken refuge in reasserting the neutral character of their work and the irrelevance of political and social considerations.[35]

This, too, according to Mulkay, is a rhetorical device to loose upon unknowing nonscientists. Yet the ideological outworkings of such a stance are there for all to observe, including other scientists. Some observers, notably Mitroff,[36] have seized upon the ambivalence of scientists – which Merton[37] saw as endemic to the institution – to demonstrate the counternormative behavior of individual scientists. These scientists see 'interestedness' and perhaps non-"communality" (i.e., secretiveness) as *rational* responses to the hostility and incessant pressures which controversial research fosters, whether it be analyses of moon specimens, which are taken to support pet theories about the origin of the moon or commitment to a viral etiology explanation of cancer.

Both of these examples entail expensive, nationally visible and funded (NASA and NCI, respectively) research. Both underscore what Orlans[38] calls the "indiscriminate advocacy of knowledge" and Salomon[39] terms "the mating of knowledge and power." Not only are scientists playing multiple roles of principal investigator peer reviewer, and science advisor, but they are instrumental in the disposition of knowledge, i.e., as advocate, popularizer and mediator vis-a-vis lay publics. Is it no wonder, then, that ambivalence arises from ambiguities regarding the cognitive and pragmatic dimensions of science? There is no consensus within the scientific community on these dimensions, yet in dealing with non-scientific publics, near unanimity must often be sounded if research efforts are to be sustained. As Nelkin suggests,

> . . . scientists engaged in research in policy-relevant areas may select research questions that are based less on disinterested judgments of intrinsic scientific merit than on organizational imperatives of their institution, or on their perceptions of social utility.[40]

To recapitulate, what Merton characterized as the scientific ethos, the universal cultural values of the scientific community, can now be seen as a class of stereotypical, ideal, *institutional norms.* Their technical counterpart, in the sense of more transitory research-specific content (theory and method) which constrains practitioners and guides the evaluation of knowledge-claims is what Mulkay[41] labeled *cognitive norms.* These norms may encompass counternormative behavior and all the ambivalence that scientists may experience over a particular research problem at a particular time. A third class of norms how-

ever, we may term *rhetorical* since they provide

> vocabularies of justification, which are used to evaluate, justify and describe the *professional* actions of scientists, but which are *not institutionalized* within the scientific community in such a way that general conformity is maintained.[42]

The import of this third class stems from scientists' selective presentations of views to support their collective research interests. Rhetorical norms, therefore, govern the articulation of an occupational ideology to non–scientific, but powerful, publics such as government agencies and congressional committees.[43]

With these classes of norms in mind, we can return to the original context for this discussion and reassess the assertion that the mission orientation of contemporary biomedicine, expecially cancer research, seems to demand a new type of research organization.

Knowledge from Money: Funding the Mission

For some, the steady rise of "contracted" projects within NIH signals a major intellectual shift, and therefore, an encroachment on biomedical research. Longo has pointed out that from 1971 to 1972, contracts in NIH increased by 47 percent but research grants by only 19 percent.[44] The concentration of this funding mode in the new mission oriented programs (46.4 percent of NCI and 27.7 percent of "heart" funds administered in 1973 through contracts) was particularly visible to this critic. Why does he find this trend alarming? Longo replies:

> Of perhaps less general knowledge are the recipients of the largest contracts . . . Of the 10 largest NIH contracts, 7 were awarded to organizations other than universities. The data are perhaps even more startling when one looks at total contracts by various organizations. Of the five largest contractors, only one is a university. Of the 12 organizations with total contracts over $3 million, only five are universities and one the National Academy of Sciences. Forty-seven per cent of NCI contracts were with *profit* making organizations in 1972. Route 70S near Bethesda, MD., is rapidly developing into a biological Route 128 composed of industrial contractors nourished by NIH. This trend, stemming from a quick-solution psychology, trends to remove research from the university and award it to industry. It remains unclear to what extent 'cost-plus research' by profit-making cor-

> porations will deprive academic scientists of funds to pursue fundamental queries . . . It is clear, however, that well-motivated scientists must provide for themselves and that they can find reasons to shift allegiances toward contract funding, especially if the squeeze is tight enough and long enough. Contract research, which is largely for product delivery or procurement purposes, has *the potential of undermining a scientist's commitment to patient, systematic and often frustrating discovery-oriented basic research*[45] (italics added).

Contracts thus represent for Longo an approach to research which conflicts with certain well established views concerning the nature of the discovery process within biomedicine. It seems unfair to his argument to simply say that it represents academic versus industrial, production-oriented, values or basic versus applied or clinical research.[46] The real problem is the nature of the discovery process and the manner in which biomedicine progresses ("cognitive norms") and whether or not the contract mechanism is a threat to this development ("rhetorical norms").

The point that must be emphasized is that the contract mechanism *did* signal a shift in organizational focus of biomedical research and NCI can legitimately be given credit for hastening the shift by emphasizing non-academic contract research. Contract research was rapidly becoming a symbol of targeted research which was executed outside the academic setting by quasi-governmental research laboratories.[47] The National Cancer Institute and its war on cancer was perhaps the single most important instrument for establishing this new organizational image for biomedical research.

The question once again arises: are the new organizational forms amenable to the structure of knowledge within the biological sciences? The concern of the scientist must center on the nature of the discovery processes within biomedical fields and how this interfaces with the goals that society would like to attain. If the policy maker is asked to heed this esoteric rationality, then perhaps the technical input–output models devised in and applied to other fields are indeed inappropriate for biomedicine. As one biochemist puts it,

> The current supports at NCI and the National Dental Research Institute for crash programs for solutions to cancers and dental caries are dangerous, in that they raise false hopes for solutions to problems for which an insufficient basis of knowledge is available. An example is the long-standing expensive Cancer Chemotherapy program which has had only slight success. It is based on the 'pill concept'; every disease can be cured if the right pill can be found.[48]

The contract, mission-oriented type of research represents for many biologists not just a violation of institutional norms, but the violation of cognitive norms, of epistemologies ("how do I know, learn, and discover within biomedicine"), which, in turn, implicate some fundamental ethiological understandings about the biological world.

The etiological conflicts seem to be a particularly resilient bone of contention, for if one acknowledges that biological existence is an evolving, systemic phenomenon, then the quest for "favorite" causal explanations (as is often latent in mission-oriented research) can lead to a kind of "sectarian" science. Pigman conveys the difficulty in the chemotherapy tradition of cancer research; the "pill concept" or some other quick technological fix can distort and perhaps hamper the development of research programs in biomedicine. NCI's discouragement of research on the viral induction of cancer in 1938 and its official reinstatement with the establishment within NCI of the Laboratory of Viral Oncology[49] are excellent illustrations of changes in etiological emphasis. Whatever the merits of this emphasis (as embodied in the Virus Cancer Program), some regard the vast sums of support for a viral etiological explanation of cancer as the epitome of sectarian science. Although a cloud of doubt hangs over the ultimate place and value of virus research in the cancer program, both its place and value would seem secure so long as the director of NCI is one with an intellectual affinity for the institute's viral oncology programs.[50]

The dangers that one presently encounters within biomedical policy is that with the switch to specific program goals and a contract orientation to research, there can also occur concomitant pressures for the adoption of specific treatments, e.g., chemotherapy, or to assume certain causal explanations, e.g., viral etiology. The switch from broadbased support of basic research to the concentrated betting on certain cures or causes, creates tensions for scientists with cognitive orientations to cancer other than those which currently enjoy popularity and political power. Given the necessity of explicit policy decisions in a national mobilization effort such as the war on cancer, there is always a danger that organization will lead to the perception of out-right "governmentalization" or the "politicization" of science[51] in the form of privileged research traditions or approaches. As James Watson has stated:

> Bad feelings about the VCP exist because there are a lot of virologists who share the same goals. The ones in the VCP were very rich. The others, who are just as good, were very poor.[52]

If, as Gustafson points out, 75 percent of all biomedical research carried out in U.S. medical schools and over 40 percent of all university research is funded

by NIH, then through shifts in policy such as that embodied in the war on cancer NIH can exert tremendous pressures on research topics.[53] Although it is difficult to decipher the distribution of pre–cancer war research proposals, Gustafson reports that "proposals to NCI now account for roughly half of all applications to NIH."[54] Scientists it would appear are following the lead of the opportunities which increased funding makes possible. It would be useful to know, of course, if researchers have actually changed their research programs or merely altered their rhetoric to fit under the umbrella of the cancer program. Further investigation of this question is sorely needed.

Conclusions

Our discussion of modifications of funding mechanisms within cancer research has suggested an interplay among normative structures by which scientists abide, but must also manipulate to protect their vested intellectual or organizational interests. By listening to criticisms by scientists one can quickly ascertain their concerns *qua* researchers. The norms to which they appeal are cognitive norms of argumentation within biomedicine and not the institutional norms which sociologists are enamored of belaboring as operative or obsolete. They may at once be both, yet this possibility is less compelling than the rhetoric scientists employ to communicate the policy-laden tensions to which their intellectual processes and products are now subject.

> Whether regarded with Panglossian optimism or Faustian foreboding, science is increasingly vulnerable to forces that intrude on its boundaries, permeate its social organization and expose its internal contradictions. Yet these forces may also bring about a more realistic awareness of the interpenetration of science and the social order.[55]

The social organization which money begets can be studied by conventional sociological means, but without an understanding of the normative structures, institutional, cognitive, and rhetorical, which guide and rationalize scientists' behavior, analysis of social organization is rendered hopelessly incomplete. Accordingly, by weighing both the discovery processes within areas such as cancer research and the criticisms which scientists have directed toward organizational tensions, sociologists of science can rethink their research tasks. The structure of reasoning can be pivotal in defining areas for study, while the search for violations of cognitive norms can translate the criticisms which scientists articulate into vital research questions about their "vocabularies of justification." For what we have here is not just Big Biology and contract research; what we have is ideology. And in Gouldner's words:

> It is one of ideology's essential social functions – of considerable cognitive relevance – to stand outside of science itself, and to reject the idea of science as *self*-sufficient or *self*-grounded. In other terms, ideology's critique of science, its refusal to let science be the only judge of itself, its public exposure of science's selfishness, . . . and the *limits* of science, mean in effect that: ideology functions as an epistemology of everyday life.[56]

For the biomedical researcher, science policy has created a new rhetorical vocabulary to vouchsafe the epistemology of their everyday science.

NOTES

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2. Daniel S. Greenberg, *The Politics of Pure Science* (New York: New American Library, 1967), p. 200.
3. Stephen Strickland, *Politics, Science, and Dread Disease* (Cambridge: Harvard University Press, 1972), p. 50.
4. The widely acknowledged architects of that policy, a coalition working from within and without the government in behalf of the Cause, were prime congressional movers John Fogarty and Lister Hill, NIH Director James Shannon, and the tireless champion in the private sector (notably the American Cancer Society), Mary Lasker.
5. The National Cancer Act of 1971, Public Law 92–218, 92nd Congress, Senate 1828, December 23, 1971. This was followed by the National Cancer Act Amendments of 1974, Public Law 93–352, 93rd Congress, Senate 2893, July 23, 1974.
6. John T. Kalberer, Jr., "Impact of the National Cancer Act on grant support," *Cancer Research* 35 (1975), pp. 473–481.
7. As Jesse Steinfeld, then Surgeon General, testified before the Senate health subcommittee deliberating on S.34 (the blueprint for the Conquest of Cancer Act), ". . . scientists are like other people, they tend to go where the funds are, where the opportunities are, and it is conceivable that if we spend an enormous amount of money in the cancer program that people who might be more productive in other programs would move to cancer programs . . ." (U.S. Senate, *Conquest of Cancer Act, 1971.* Hearings before the Subcommittee on Health of the Committee on Labor and Public Welfare, United States Senate, Ninety-second Congress (Washington, D.C., 1971)).
8. Alvin Weinberg, "The coming age of biomedical science," *Minerva* 4 (1965), pp. 3–14.
9. U.S. Senate, *op. cit.*
10. Strickland, *op. cit.*, pp. 236–290.
11. U.S. Senate, *op. cit.*, p. 231.
12. *Ibid.*, p. 232. The National Heart and Lung Institute did not have long to wait, however, before they would also be singled out for special funding. In 1972 the President signed the National Heart, Blood Vessel, Lung, and Blood Act. See Barbara J. Culliton, "Biomedical research (II): Will the 'wars' ever get started?" *Science* 181 (7 September 1973), pp. 921–925.

13. This appears to be a particularly difficult request to make of sociologists. For as Gernot Bohme ("The social function of cognitive structures: a concept of the scientific community within a theory of action," in K. D. Knorr, H. Strasser, and H. G. Zilian, eds., *Determinants and Controls of Scientific Development* (Dordrecht; Boston: D. Reidel, 1975), p. 215) has argued: "What sociology of science has generally failed to do is to account for the primacy of cognitive orientation with regard to the social organization of science. In contrast to this, the organization of science was constructed solely on the basis of motivational orientation: thus Storer took the demand for creativity and competent reaction to be fundamental, and Hagstrom the system of sanctions. To be sure, the organization on the level of motivation is a reality, but it presupposes organization on the cognitive level."
14. John Gardner, "The government, the universities, and biomedical research," *Science* 153 (30 September 1966), p. 1602.
15. Strickland, *op. cit.*, p. 189.
16. By the 1970's it was clear that the National Cancer Institute was destined to be reorganized in accordance with a new mission–oriented mandate. When Congress established the Special Virus Leukemia Program (1965) with an appropriation of $10 million, it became obvious what Congress had in mind. They wanted results! And to get results this Program engaged in more *contract* research than had been the custom of any NIH agency (see Kalberer, *op. cit.*, pp. 475f). Such contract research demanded, in turn, a new type of planning, and the operations research strategies that had worked for other areas of science were once again called upon for assistance. Carl G. Baker, then Director of NCI, Frank Rauscher, the newly-appointed Chairman of the Special Virus Leukemia Program (later called the Virus Cancer Program), and Louis M. Carrese, a systems management specialist, set out to develop a rational basis for mission-oriented, therefore contract–oriented, cancer research. See C. G. Baker, L. M. Carrese, and F. Rauscher, "The special virus-leukemia program of the National Cancer Institute: Scientific aspects and program logic," in R. N. Fiennes, ed., *Some Recent Developments in Comparative Medicine*, Symposia of the Zoological Society of London, 17 (London, 1966); L. M. Carrese and C. G. Baker, "The convergence techniques: a method for the planning and programming of research efforts," *Management Science* 13 (1967), pp. B420–B438; Culliton, *op. cit.* Although they clearly recognized the limits of organizational theory when it was applied to biomedical research programs, and although they sought to remain as flexible as possible, they were nevertheless constrained to make certain assumptions limiting the scope of the program. For example, "The main assumption of working hypothesis on which the over-all program is based is that at least one virus is an indispensable element for the indication (directly or indirectly) of at least one kind of human leukemia (including lymphoma) and that the virus persists in the diseased individual" (Baker *et al., op. cit.*). The emphasis on the viral ethiology of cancer, in particular the importance of RNA viruses, had been organizationally blessed. Scientific "rewards" were now to be defined in terms of various lines of research which would adopt this assumption and progress toward the cancer cure. Contracts, it would seem, were often stigmatized by the academics because they removed research from the independently motivated researcher. By the 1970's, when medical researchers began expressing their concerns about the National Cancer Art, the organizational die had already been cast.
17. U.S. Senate, *op. cit.*, p. 102.
18. *Ibid.*, p. 391.
19. *Ibid.*, p. 392.
20. *Ibid.*, p. 196.
21. Quoted in Joseph Hixson, *The Patchwork Mouse* (Garden City, N.Y.: Doubleday, 1976), pp. 178–179.

22. Barbara J. Culliton, "Kennedy hearings: Year-long probe of biomedical research begins," *Science* 193 (2 July 1976), p. 33.
23. Gene M. Lyons, *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century* (New York: Russell Sage Foundation, 1969).
24. Michael J. Mulkay ("Norms and Ideology in science," *Social Science Information* 15 (1976), pp. 637–656) stresses this very point in recognizing that the rhetoric of scientists' pronouncements or "vocabularies" vary with the audience they are addressing. Such a strategy serves multiple purposes, e.g., maintaining distance between the scientists-expert and the lay public, and promulgating the search-for-truth ideology as a rationale for decrying impediments to the flow of research dollars.
25. U.S. Senate, *op. cit.*, p. 393.
26. Quoted in Hixson, *op. cit.*, p. 161.
27. Daniel S. Greenberg, "Cancer: Now, the bad news," *The Washington Post* (19 January 1975).
28. Barbara J. Culliton, "Virus Cancer Program: Review panel stands by criticism," *Science 184 (12 April 1974), pp. 143–145.*
29. *Ibid.*, p. 143.
30. *Ibid.*, p. 144.
31. *Ibid.*, p. 144.
32. Perhaps the most devastating appraisal by the Zinder committee concerned the centralization of the (S)VCP: "It was only natural that when the SVCP was formed [initially to explore the possible role of adenoviruses in malignancy], a small group of investigators was involved – an 'in group.' It now represents a somewhat larger 'in group' of investigators. Administratively its procedures lack vigor, are apparently attuned to the benefit of staff personnel and are full of conflicts of interest. Because the direct targets have become fuzzy since 1964, although the available funds have continued to grow, the program seems to have become an end in itself, its existence justifying its further existence. In doing so, it is eroding what is good in both the grant and contract mechanisms, a fact which may account for the widespread antipathy to SVCP in the scientific community" (quoted in Hixson, *op. cit.*, p. 132).
33. Robert K. Merton, "Science and technology in a democratic order," *Journal of Legal and Political Sociology*, 1 (1942), pp. 115–126; Bernard Barber, *Science and the Social Order* (Glencoe, Ill.: Free Press, 1952).
34. Clearly, attitudes toward funding policy cannot be reduced to comparisons of the relative funding levels of the various Institutes. Some comparisons, however, are instructive. Even though there was a fairly stable number of grants reviewed during the late 1960's and the approval rate was increasing slightly, the actual award rate was decreasing dramatically. This condition was created by changes in the rate of increase of available funds, shifts in the proportion of renewals to new grants, and the increase in the average cost of research grants. A tense situation was brewing *within* all of the Institutes. Kalberer (*op. cit.*, p. 478) observes that "In the case of NCI, award rates started to fall off dramatically beginning in 1968 as a result of the leveling off of Congressional appropriations. Consequently, there was a steady downward trend in the percentage of traditional grants awarded, particularly new grants; in 1970 the Institute reached its all time low level, awarding only 30% of approved grants. With passage of the Act of 1971 this trend was reversed. Within the last 4 years, at least 50% of all approved new applications have been awarded." Further (*Ibid.*, p. 479), "The halcyon years, prior to 1964, when NIH, including NCI, was able to fund more than 90% of all approved applications, have passed." Relative deprivation could be perceived, thus, not only between Institutes but also *within* Institutes as funding capabilities changed in the post war era. The new wars on cancer and heart disease did not occur in a vacuum; the basis for a feeling of deprivation was already present. What made the situation intolerable to many, however,

was that they were being deprived *more* than others. It is this phenomenon of relative deprivation which Rauscher ("Research and the national cancer program," *Science* 189 (11 July 1975), p. 118) was trying to defuse. But even while Rauscher proposed the independence of the NIH and NCI budgets, Kalberer (*op. cit.*, p. 477) notes that "As a result of the decrease in budgets of the other institutes, NCI is making every effort to fund outstanding applications assigned initially to other institutes but not payable because of lack of funds." Recognition of the interdependence of the Institutes seemed to obtain more in practice than in administrative theory.

35. Dorothy Nelkin, "Changing images of science: new pressures on old stereotypes," *Newsletter* 14 of the Program on Public Conceptions of Science, Harvard University (1975), pp. 21–22.
36. Ian I. Mitroff, "Norms and counter-norms in a select group of the Apollo moon scientists: a case study of the ambivalence of scientists," *American Sociological Review* 39 (August 1974), pp. 579–595.
37. Robert K. Merton, "The ambivalence of scientists," in N. Kaplan, ed., *Science and Society* (Chicago: University of Chicago Press, 1965).
38. Harold Orlans, "Neutrality and advocacy in policy research," *Policy Sciences* 6 (1975), pp. 107–119.
39. J.-J. Salomon, "The mating of knowledge and power," *Impact of Science on Society* 22 (January–June 1972), pp. 123–132.
40. Nelkin, *op. cit.*, p. 26.
41. Michael J. Mulkay, "Some aspects of cultural growth in the natural sciences," *Social Research* 36 (Spring 1969), pp. 22–52.
42. Mulkay, *op. cit.*, p. 654.
43. The recent recombinant DNA controversy and NIH deliberations on security guidelines for university laboratories have brought representatives of the scientific community into encounters with a formidable "external" power–city councils determined to minimize the biohazards in *their* community.
44. Lawrence D. Longo, "Some problems facing biomedical research," *Federation Proceedings* 32 (1973), p. 2080.
45. *Ibid.*
46. See Gerald Gordon and Sue Marquis, "Freedom, visibility of consequences and scientific innovation," *American Journal of Sociology* 72 (September 1966), pp. 195–202; Stephen Cotgrove and Stephen Box, *Science, Industry and Society: Studies in the Sociology of Science* (London: Allen and Unwin, 1970); Julius H. Comroe and Robert D. Dripps, "Scientific basis for the support of biomedical science," *Science,* 192 (9 April 1976), pp. 105–111.
47. In the case of NCI policy, contracts did not simply signify an administrative technicality which allowed academic researchers to conduct their research as they had in the past under the grant system. As Longo (*op. cit.*) pointed out, NCI was heavily committed to funding non-academic research. Some of the largest contractors with NIH are Litton Industries (22 contracts in 1972 totalling $16.5 million), Microbial Associates (12 contracts in 1972 totalling $7 million), and Flow Laboratories (9 contracts in 1972 totalling $4 million), all private laboratories which are involved in NCI cancer research.
48. Ward Pigman, "Government support of biomedical research," *Federation Proceedings* 32 (1973), p. 1733.
49. This did not occur until 1961; thus it took NCI 23 years to change its mind and effect a new policy.
50. With Frank Rauscher's retirement as NCI director, a shift in emphasis to environmental carcinogen research and a corresponding deemphasis on viral oncology is probable. See Barbara J. Culliton, "Arthur Canfield Upton: New director of the NCI," *Science* 197 (19 August 1977), pp. 737–739.

51. One of the difficulties with analyzing the politicization of science is that all the readily available examples occur in political situations which are easily condemned, e.g., the case of T. D. Lysenko (see Zhores A. Medvedev, *The Rise and Fall of T. D. Lysenko* (New York: Columbia University Press, 1969)). It would seem, however, that even the most gradual policy shifts can subtly politicize a research problem by establishing a reward system which is out of touch with the present scientific realities. See Joseph Haberer, *Politics and the Community of Science* (New York: Van Nostrand Reinhold, 1969).
52. Quoted in Culliton, *op. cit.*, p. 144.
53. Thane Gustafson, "The controversy over peer review," *Science* 190 (12 December 1975), p. 1060.
54. *Ibid.*, p. 1063.
55. Nelkin, *op. cit.*, p. 27.
56. Alvin W. Gouldner, *The Dialectic of Ideology and Technology* (New York: Seabury Press, 1976), p. 36.

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